THE RHYTHM METHOD: TWO INDIAN EXPERIMENTS

By C. P. BLACKER

PART I *

BY most non-Catholic people in this country who are concerned in the family planning movement, the rhythm method is regarded with little favour. It is held to be widely unacceptable and, by most couples, impracticable; and when found practicable it is declared to be unreliable.

The method is also said to run counter to nature in that it prohibits intercourse during the days of the cycle when it is most desired by the woman. The human female is believed to conform with the mammalian rule that desire is greatest at the time when the chances of conception are greatest, her psychology being adjusted to her physiology. Hence the rhythm method is commonly dismissed with something like contempt.

Importance in the World of Rhythm Method

Yet from the standpoint of world population the matter is important. The rhythm method is, for practical purposes, the only one permitted by the Roman Catholic Church to which about 430 millions owe allegiance. The South American continent, where numbers are increasing faster than in any other continent, is almost wholly Catholic. Several European countries are likewise placed and others contain united and sometimes powerful Catholic minorities. These minorities may, as in Canada, have a higher fertility than the non-Catholic majority.

The value (in terms of practicability and effectiveness) of the rhythm method is therefore a matter of high general importance. To individual Catholic parents the matter has the same significance as to parents of other denominations who want to plan their families. And there is the same scale of needs. At one end is the healthy couple who wish to space their children; at the other the woman who is told that she may die if she again becomes pregnant.

In these circumstances it is strange that the Roman Catholic Church has not sponsored an impartial and comprehensive inquiry into the rhythm method. Many books have been written about it by Catholic authors—several jointly by doctors and priests. These books vary within fairly wide limits in approach and language; but in none is there a record of a scientifically designed and controlled inquiry.

For these reasons the Indian investigations known as the Ramanagaram and Lodi experiments deserve to be closely examined. The final report† is now available in two cyclostyled volumes.

Briefly, these two volumes show that the experiments were failures in the sense that they provided but incomplete answers to the rather searching questions which they were designed to answer. But important experience was nevertheless gained and some valuable lessons were learned. Indeed, the enterprise as a whole reflects much credit on the Indian Government and on the persons concerned with the inquiries. Certain features (mentioned below) of the *Report* suggest that both investigations were somewhat abruptly terminated when it became clear that results on a sufficiently comprehensive scale could not be obtained. Indeed, if the inquiries had been carried but a little further a picture clearer in certain important details might have emerged.

* Part II will appear in the October number of the EUGENICS REVIEW.

† *Final Report on Pilot Studies in Family Planning.* Two volumes with 36 Appendices. Issued by the World Health Organization (WHO) Regional Office for S.E. Asia. New Delhi. September, 1954. No price given.

Background of the two Experiments

The two experiments were the outcome of certain general considerations advanced in India's first Five-Year plan (1947-1951) and of some specific proposals contained in her second Five-Year plan (1952-1956). Shortly after acquiring independence in 1947, the designers of the first plan recognized how the rapid growth of India's population could impede the aims of the miscellaneous economic, health and welfare schemes which were being prepared with a view to raising the standard of living.

> The pressure of population in India, they said, is already so high that a reduction in the rate of growth must be regarded as a major desideratum. To some extent improvement in living standards and more widespread education, especially among women, will themselves tend to lower the rate. *But positive measures are also necessary for inculcation of the need and techniques of family planning.* (My italics.)

Such were the considerations in the minds of the designers of the first plan. The proposals advanced in the second plan were at the same time simple, definite and comprehensive. They were to :

(*a*) obtain an accurate picture of the factors contributing to the rapid population increase in India;

(*b*) discover suitable techniques of family planning and devise methods by which knowledge of these techniques can be widely disseminated; and

(*c*) make advice on family planning an integral part of the services of Government hospitals and public health agencies.

In response to these proposals, the Indian Ministry of Health drew up a plan which, in effect, posed certain questions which were concerned in different ways with principles and methods. These questions, which relate to specifically Indian conditions, can be summarized under six headings which are most conveniently arranged in the following order :

(1) How far is the *principle* of family planning still alien to or unacceptable by different Indian populations? Do cultural or ethical difficulties still block the way? For example an attitude of fatalism—that fertility is predestined and outside human control; or the feeling that attempts at such control are impious or hubristic and therefore likely to be followed by retributive misfortunes or to bring bad luck.

(2) If the principle is unacceptable, how can it be made acceptable by education?

(3) If the principle is found to be, or becomes, acceptable, what *methods* are:

most acceptable to men and women of different creeds, cultural patterns and degrees of education; and
most practicable in the living conditions of village, small town or city?

(4) How far are *methods* which are culturally acceptable and domestically (under prevailing living conditions) practicable, also *effective* in regulating conceptions according to plan?

(5) Can effective *methods* be made more acceptable and practicable by education or by communal or state action?

(6) If none of the existing methods of family planning satisfactorily meets the above requirements of being culturally acceptable, domestically practicable, and effective in regulating conception, can *new methods* which better meet these requirements be discovered by appropriate research?

These important and necessary questions, some of which are more relevant to undeveloped than to developed countries, can only be answered by field studies, social and administrative experiments, and research.

We are told that, early in 1951, the Indian Ministry of Health decided to inaugurate studies in the use and effectiveness of the rhythm method among village and urban populations. The rhythm method was selected by the Government because (in the words of the *Report*) :

(*a*) its practice would involve no expense to the couples;

(*b*) its practice would not necessarily require medical supervision; and

(*c*) it appeared to be consonant with the Indian traditions of self-control and, in particular, was acceptable to Gandhian thought.

This last condition is culturally important in India and should not be overlooked by westerners. The report says that " a major assumption in propagating the rhythm method in India is that the cultural background of its people has made them accustomed to follow some pattern of abstinence for socially accepted reasons ".

In August of the same year (1951), the Indian Government, through its Health Ministry, applied to the World Health Organization (WHO) for help in the organization and conduct of field studies in the rhythm method. The Government wisely asked for the assistance, as an organizer of the studies, of Dr. Abraham Stone, a leading American authority on problems of birth control and fertility who is well known to readers of this REVIEW ; they also asked for two field workers to assist in the administration of the experiments.

These were Mrs. Taylor (formerly Miss Mary Langford) and Miss Margaret Snyder. The first took charge of the Ramanagaram, and the second of the Lodi experiments. The United Nations was indirectly concerned : at the request of the Indian Government it made available the services of such distinguished men as Dr. C. Chandrasekaran and Dr. Sten A. W. Wahlund whose memoranda on the three applications of the rhythm method (mentioned below) are given prominence in the two volumes. Dr. Wahlund was present in India on two technical assistance missions for an aggregate period of about seven months during the years 1952 and 1953. Dr. Kingsley Davis and Dr. P. K. Whelpton, both eminent demographers, were also concerned.

Five *areas* were originally considered as suitable for the experiments. These were situated in the north (Delhi state and New Delhi) east (Bengal) and south (Mysore and Madras) of India. Two of these areas were finally selected. Ramanagaram, in the state of Mysore, is situated in the southern extremity of the Indian peninsula ; Lodi colony, some eleven hundred miles north of Mysore, is on the outskirts of New Delhi. As will be seen below, the populations in these two areas were culturally different and the designs of the two experiments were also different.

The *aims*, drawn up beforehand, of the two experiments were somewhat ambitious. In the words of the *Report* :

> Both studies have as their common aims the measurement of:
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> (*a*) the acceptability of the rhythm method;
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> (*b*) the effectiveness of the rhythm method in reducing the number of pregnancies among those who undertake to use it. The Ramanagaram study has as an additional aim:
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> (*c*) the effectiveness of the rhythm method in reducing the birth-rate of the community.

It will be seen below that the data which, by the time the experiments were discontinued, had been laboriously and painstakingly accumulated were quite insufficient to provide an answer to the last of these questions. The answer to the second, though of little statistical value, could have been formulated with more accuracy if, after the closure, four cases of suspected pregnancy had been followed up.

In the *Conclusions* of the *Report*, these three questions are not formally raised and answered. Nevertheless the answers are there, and will be considered below.

It was mentioned above that a major assumption underlying the two experiments was that the cultural background of India's peoples had accustomed them to follow some pattern of abstinence for socially accepted reasons. The first and perhaps the foremost question was whether this " pattern of abstinence " was such that an acceptance of the rhythm method, which calls for regular periods of abstinence, could be built upon it. We are told that, in order to throw light on these patterns, an *ad hoc* survey was conducted in October 1952, shortly after main studies at Ramanagaram and Lodi had been begun.

I heard Dr. Chandrasekaran give a preliminary report on the findings of this inquiry at the third International Conference on Planned Parenthood held in Bombay at the end of 1952. In a discussion of the " cultural component " of sexual abstinence in Indian life, Dr. Chandrasekaran said:*

> Avoidance of coitus associated with religious festivals and fast days was quite common in both Ramanagaram and Lodi Colony. Fifty per cent of the persons interviewed reported such avoidance. The phase of the moon plays an important role here. New moon dàys, full moon days and *Ekadashi*, i.e. the eleventh day after the new or

* *Proceedings of Third International Conference on Planned Parenthood, 1952*. Pp. 73-79. Obtainable from International Planned Parenthood Federation, 69 Eccleston Square, London, S.W.1. Price 10s. 6d.

the full moon, were frequently mentioned in Ramanagaram. Specific days of the week were mentioned by some in this area, especially Sunday, Monday and Saturday. The days when a man has a shave and a bath, days of the sowing of the fields, days of solar and lunar eclipses were also mentioned in some cases.

The reasons for avoidance in Lodi Colony related either to the phase of the moon or to well-known festivals. The number of days of avoidance for religious reasons mentioned by individuals ranged from two to 120 per year in Ramanagaram and from one to seventy-nine in Lodi Colony; the median was twenty-four days in Ramanagaram and nineteen days in Lodi Colony. . . .

In both areas abstinence from coitus during the wife's pregnancy was reported. On an average this abstinence began after about five months in Ramanagaram and six months in Lodi Colony. Abstinence during lactation was also reported in both areas. In Ramanagaram 80 per cent of the cases reported such abstinence for six months or more; in Lodi Colony the period of abstinence seldom exceeded three months.

Dr. Chandrasekaran further pointed out in 1952 that traditional beliefs tend to result in coitus taking place at the time when conception is now known to be most probable. This matter is discussed in the *Report*. We are there told that " an important cultural practice which contributed to the difficulties of teaching the Rhythm Method was the avoidance of coitus for at least eight days after the onset of menstruation. This practice has been found to be practically universal among the village couples. In a few cases this avoidance continues for 12 to 15 days and in one case for 18 days ".

Commenting on this practice, Dr. Chandrasekaran pointed out at Bombay that :

> Whatever the origins of the tradition, it presents the inescapable fact that the rhythm method can prove effective for many people in these communities only in so far as they can be brought to make certain revisions of their existing patterns of sexual union as they relate to menstruation.

Dr. Chandrasekaran was careful to point out " that these studies do not answer all the questions raised by India's cultural diversity with respect to human reproduction. Additional studies will need to be undertaken in various parts of the country ". He was also careful to stress that the views expressed in his paper were personal and did not commit in any way the United Nations which had made his collaboration in the inquiries possible.

It will be seen below that, despite the existence in both places of cultural patterns favourable to abstinence, the results of the Ramanagaram and Lodi experiments suggest that the rhythm method often makes excessive demands on the willingness of Indian husbands to practise abstinence for the necessary periods.

Three Adaptations of the Rhythm Method

Three methods, a routine method and two modifications, were tried.

(*a*) The Routine Method. Here information about three and preferably six of the woman's cycles is necessary.

(*b*) The One-Cycle rule. Information about one cycle only is here necessary.

(*c*) The Omnibus rule. Applicable when nothing is known of the woman's cycle.

The object of all three methods is to inform the woman which are the days in her cycle when conception is likely to occur (here called the " baby days ") and which are those when conception is unlikely (here called the " no baby days", " safe days", or the " safe period").

The rhythm method, when used for purposes of avoiding pregnancy, requires abstention from intercourse during the " baby days".

For those unfamiliar with the method, a brief outline of its theoretical basis, as set out in this *Report*, may be helpful. Ovulation normally occurs between the twelfth and sixteenth day after the first day (or onset) of menstruation, here and throughout the *Report* called " onset day". It is obvious that on the days in the middle of the cycle, immediately before and immediately after ovulation, coitus is likely to be followed by conception. These are the " baby days", during which abstinence must be practised. In the calculation of the " baby days", allowances have to be made

for two factors which are relatively unvarying and for one which is variable. The two relatively unvarying factors are the length of life in the woman's genital tract of the unfertilized ovum (about two days), and the length of life of spermatozoa in the same genital tract (about three days); the variable factor is the length of the woman's cycle, which is mainly determined by when ovulation takes place.

The woman's cycle can, for this purpose, be divided into two periods; the first (the pre-ovulation phase) is the period elapsing between the onset day (above defined) and the day on which ovulation takes place; the second period (the post-ovulation phase) is the period elapsing between the day of ovulation and the next onset day. Variations in the length of the total cycle are more likely to be produced by irregularities in the first of these two periods (the pre-ovulation phase) than in the second (the post-ovulation phase). Indeed it will be seen below that one of the above-mentioned modifications of the rhythm method, the One Cycle Rule, is based upon the assumption that the last seven days of the post-ovulation phase (the seven days before onset day) can definitely be counted as " safe". By then ovulation will almost certainly have occurred and the ovum will no longer be fertilizable.

The central problem, then, of the rhythm method is how to calculate which are the woman's " baby days " (during which abstinence must be practised) while making due allowances for possible variations in the length of the expected cycles.

It follows that, in theory, the " no baby " days, or " safe days " occur at two different times in the woman's cycle, namely before and after the " baby days". The " early safe days " (before the baby days) begin with the onset day; the first three or four are taken up with menstruation and the number of the remainder varies with the length of the established cycle. The " late safe days " follow the baby days and end the day before onset. The shorter the cycle, the fewer of these " early safe days", the longer the cycle, the more of them. The late safe days do not vary.

(*a*) *The Routine Method*

This method is based on the observation of the length of the woman's cycles over a preliminary or " observation " period of six months. No advice as to how to make use of the rhythm method is imparted during the first three months of this observation period. But after four onset days (three cycles) have been recorded preliminary " rhythm advice" is given. At this stage about twenty-one days in the cycle are flagged as " baby days " —a long period calling for a considerable exercise in abstinence.

Three months later when seven onset days are recorded (covering six cycles) the rhythm advice is revised and if possible fewer baby days are assigned. The need for this rather long period of observation is a definite drawback to the rhythm method—a drawback which is not shared by the usual appliance methods. It will be seen below that many women became pregnant during this " observation period " and that others lost interest.

In the course of the observation period lasting six months, seven " onset days " are recorded and the duration in days of the longest and shortest of the six cycles is noted. If the difference between the longest and shortest cycle exceeds eight days (being nine or more days) the rhythm method is not recommended. This condition excludes a proportion of women and is another drawback to the rhythm method compared to others.

The longest and shortest of the six cycles having been determined the procedure is to deduct eighteen from the number of days of the shortest cycle and eleven from the figure for the longest cycle. Suppose a woman's longest cycle were thirty-three days and her shortest twenty-seven days—a difference of six days. Twenty-seven minus eighteen gives nine; thirty-three minus eleven gives twenty-two. This woman's " baby days " are from the 9th to the 22nd day of her cycle—in this case fourteen days. The remaining days are " no baby days " or " safe days". If this woman's menstrual period lasted four days, the eight days after onset being safe, she would have four

" early safe days " during which intercourse could take place immediately after menstruation has ceased ; she would also have in a twenty-nine day cycle about eight " late safe days " before it again began—in all twelve " safe days " against fourteen " baby days". The smaller the variation in the length of the cycle the more numerous the " safe days " and the fewer the " baby days". Thus, if the woman's six cycles, as noted during the " observation period " of six months, had all lasted thirty days, thirty would be the figure from which both eighteen and eleven would be subtracted. Thirty minus eighteen gives twelve and minus eleven gives nineteen. Her " baby days " would run from the 12th to the 19th day inclusive—eight days. The " safe days " would be correspondingly more numerous. Briefly the longer the cycle and the less it varies the fewer are the " baby days " and the longer the safe period. The gain in length is effected by an increase in the post-menstrual or early safe days.

(*b*) *The One-Cycle Rule*

This application of the rhythm method is used when it is regarded as urgent that immediate guidance should be imparted and when the duration of but a single cycle is known. How does the woman calculate the probable length of the expected cycle on the basis of knowledge of a single past cycle ? She does it by adding the figure 29 to the number of days in the single recorded cycle and dividing the total by two. Twenty-nine days is the average length of the cycle. Thus a woman whose single observed cycle covered twenty-five days would have an expected cycle of twenty-seven days (25 plus 29 equals 54 ; 54 divided by two is 27). The One-Cycle rule was tried in the fifteenth village (Bannikuppe) in the area of the Ramanagaram experiment which included fourteen " experimental " and sixteen " control " villages (see below). The One-Cycle rule is only applicable when the single known cycle is not less than twenty or more than forty days in length. If the cycle falls outside these limits, the " rule " is inapplicable.

The " rule " fixed seven pre-menstrual days as " safe". The fifteen preceding days are " baby days". Thus, of the twenty-one days *before* onset day, the first fifteen will be " baby days " and the last seven " safe days". Hence, the longer a woman's cycle, the more *post-menstrual* or " early " safe days will be available (the *pre*-menstrual or " late " safe days being fixed at seven). A woman with an established twenty-one day cycle would have no " early " safe days ; one with an established cycle of thirty days would have nine such days (including those taken up by menstruation).

The One-Cycle rule is held to be less dependable than the routine method of calculating the " baby days", but it has the advantage of shortening the observation period from six months to one.

(*c*) *The Omnibus Rule*

This " rule", which is less reliable than the other two—Dr. Wahlund suggests that it covers 90 per cent of cycles—is applicable to women as to whose previous cycles nothing is known. It assumes that the fifteen days elapsing from the 8th to the 22nd days after " onset " (the first day of menstruation) are " baby days " and that the other days in the cycle are " safe days". A small-scale test of the Omnibus rule was made on an illiterate but highly orthodox colony of sweepers and peons at Aliganj, near Lodi (see below).

One of the advantages of the routine over the other two methods is that, for women with regular cycles, the number of " baby days " can be reduced. The method is, so to speak, elastic. As mentioned above, a woman who, for example, could report six unvarying cycles of thirty days would have but eight " baby days " in each cycle ; about a week's abstinence would be demanded. But both the One-Cycle and Omnibus rules invariably prescribe fifteen baby days. Hence about a fortnight's abstinence is exacted. This long period of abstinence was found difficult by many couples. Thus in Aliganj, where the small experiment in the Omnibus rule was tried, the provisional conclusion was reached that " few couples will adopt the Omnibus

rule because of the long period of abstinence which is required ".

Instructions to the Couple

We are told that, of the 811 " willing to learn " couples observed in the fourteen " experimental " villages of Ramanagaram, only 155 men (19 per cent) and nineteen (2 per cent) of women reported that they could read. Their powers of calculation were correspondingly limited. Simple methods of instruction had therefore to be devised in order to secure accuracy in the recording, over six months, of the seven onset days the precise dates of which were needed before " rhythm advice " could be finalized.

An ingenious system of beads (in the form of necklaces) was devised by Dr. Stone and offered to all the co-operating couples in the Ramanagaram experiment. The beads were in three colours. A single red bead appropriately fixed the onset day. Green beads indicated the " no baby " or safe days; black beads the " baby days " when abstinence was required. If the beads were rejected, calendar cards were offered. These were more acceptable to the men (among whom there was more literacy) than to the women. Some couples rejected both beads and calendars. To these oral instructions were given.

The use of the necklaces of beads presented difficulties which give an idea of the problems presented by educationally backward populations. Many women believed that the beads had magical qualities and that the mere act of manipulating the beads protected them against pregnancy. In the words of the *Report* " a major teaching problem was convincing the woman that merely moving the beads daily did not protect her from pregnancy ". Other objections were that, if handled during the days of menstruation, the beads would become polluted; and that during certain religious festival days they were unclean and should not be retained in the house. We are told that, on these occasions several women returned their beads to the workers in the experiment. The *Report* adds:

> Both the women who accepted the beads, and the men who accepted the calendar cards have claimed to be too busy to check the item daily. Most of the couples resorted to weekly checking. A number of couples had no place in their houses for keeping their aids safely from the curious eyes of other members of the household. One woman left her beads next door with a neighbour.

Such difficulties though doubtless discouraging to the organizers of the experiment are revealing. They well illustrate the need for the close study of cultural and domestic factors before embarking on large-scale programmes of national scope.

The Ramanagaram Experiment

Of the two experiments this was the most ambitious and much the most difficult. It was also the most generously staffed. We are told that at least fifty workers were employed in it for varying periods. The first batch of thirty-one field and four office workers began work in May 1952. Many of the difficulties arose from the scattered distribution of the rural population in small villages and hamlets each of which had to be frequently visited. The *Report* tells us that " only a few of the villages are located on a 'pukka' (paved) road. The others are approachable by 'kutcha' (dirt) bullock track roads. The majority of the hamlets are reached only by cart tracks or footpaths ". The workers were confronted with serious transport difficulties during certain stages of the inquiry.

Two reasons are given for selecting Ramanagaram as a suitable locality for this experiment. The first is the fact that in 1936 the Rockefeller Foundation had established there a Health Training Centre which may have familiarized the population with health services. Indeed it seems possible that as a result of this past experience the experimental population of Ramanagaram was more amenable to new ideas than other rural populations might have been. The second reason was the availability of census data from Mysore's State Census Office and from certain population studies previously conducted by Dr. Chandrasekaran.

The design of the experiment was ambitious. Thirty-one villages were included. Of

these, fourteen were " experimental " and sixteen " control " villages. A fifteenth " experimental " village (Bannikuppe) was later added for the special study of the One-Cycle rule. The *Report* gives some particulars of the demographic, occupational and climatic conditions of the experimental and control areas. We are told that the population of the fourteen experimental villages numbered 7,564. The birth rate in these villages during the four years 1950-1953 was high ; it varied between 45 and 54 per thousand. Hence the number of births varied during these years between about 330 and 405. These figures have a bearing on the third question to which the experiment was expected to provide an answer namely " the effectiveness of the rhythm method in reducing the birth rate of the community " (see above).

There is little comparison in the *Report* between the findings in the " experimental " and " control " villages. Indeed, little is said about the control experiment.

A feature in which the Ramanagaram contrasted with the Lodi experiment was that in the former both husbands and wives were interviewed ; in the latter, only the wives. In Ramanagaram, lady workers interviewed the wives and men workers the husbands, if possible on the same day—not an easy condition to translate into practice. The WHO supervisor (Mrs. Taylor) and the workers had to surmount many obstacles and frustrations. Two may here be mentioned, namely " the intensity of the untimely criticisms from various sources [which] at times hampered the efforts of the workers to conduct the study objectively" ; and the gradual realization of the limitations of the rhythm method. This difficulty is further considered below.

The disappointing course of the inquiry may now be generally indicated. An elaborately planned and ambitious investigation, from which definite and substantial results were clearly expected, was slowly found to dwindle in scope and possibilities. As the inquiry progressed, increasing numbers of couples fell out or were dropped as unsuitable and the number of couples whose experiences could yield the desired information dwindled. By miscellaneous processes of erosion, a respectably sized mountain slowly shrank to the dimensions of a mound. To 112 women only was the rhythm method eventually taught ; and of these but thirty-nine used it sufficiently regularly for its reliability as a birth control method to be assessed. Indeed, the erosion of the mountain proceeded so far that it is easy to understand why the experiment was abruptly and perhaps prematurely terminated.

It is notorious that all birth control methods are difficult to assess according to scientifically acceptable standards of accuracy in respect of their two most relevant features, namely acceptability and effectiveness (reliability). Of no method, however, are the difficulties of inquiry and assessment greater than of the rhythm method. In what follows, attention will be drawn to difficulties in the Ramanagaram experiment which, being specific to the rhythm method, would not have arisen from other methods if these had been under investigation instead of the rhythm method.

Preliminary Stages

The preliminary stage of the inquiry included the preparation of a house list for each village and its surrounding hamlets, and the conduct of *household surveys* of the houses in the villages of the experimental and control areas (every other house was surveyed in the control area). Detailed personal particulars of all inmates were obtained in these household surveys which included the reproductive histories of women who were, or had before been, married. In this way it was ascertained how many households contained couples who could be included in the inquiry—i.e. couples who were sexually active, the wives being under forty.

An " *Attitude Survey* " was then undertaken : this was designed to find out how many couples wanted to learn a method of family planning. As above mentioned, both husbands and wives were questioned about this matter by male and female workers respectively, if possible on the same day.

Case cards (a facsimile is reproduced in volume 2 of the *Report*) were then filled in. These case cards, on which was recorded all the relevant preliminary information, were designed for " maintaining a running record of the various contacts with the co-operating couple". The information on these cards provides the statistical material for the *Report*. A final feature of the preliminary stage of the inquiry were two *ad hoc* studies, obviously necessary in the general context of the inquiry, which were designed to ascertain whether the village women recognized colours (cf. the necklaces of beads), whether they knew the days of the week, how they reckoned days, weeks and months, and what counting system they used.

The figures which emerged from the preliminary stage of the experiment were as follows :

> The fourteen experimental villages contained 1,600 *households*; these households contained 1,301 *couples*, the wives being under forty.
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> Of these 1,301 couples, attitude surveys were completed in respect of 1,088 *couples* and abandoned in respect of 213 couples. The reasons for abandonment had nothing to do with the rhythm method. Inability to interview both partners (for which there were several causes) and non-consummation of the marriage were mainly responsible.

The results of the Attitude Survey (which covered 1,088 couples) showed that one or both partners of *811 couples* (75 per cent) were willing to learn a method of birth control, and that 277 (25 per cent) were unwilling. The reasons for this unwillingness again had nothing to do with the rhythm method. A large fraction—103 women (37 per cent)—had no surviving children. A still larger fraction—127 women (45 per cent)—had had children but wanted more ; among these were twenty-three who had had no male children. Five women gave as a reason that they wished to obey their elders, and another five that children were gifts of God.

Instruction and Application Stages

So much for the preliminary stage of the inquiry which included the attitude surveys : these produced the highly encouraging finding that three-quarters of the couples interviewed expressed willingness to learn a birth control method—a proportion which was closely matched by the Bannikuppe and Lodi couples (see below). The preliminary stage was followed by the *Instruction and Application stages* of the experiment. The results here were far from encouraging. Indeed, the outcome was a most drastic whittling down of numbers.

Of the 811 couples who were willing to learn a birth control method, the wives of 247 couples (30 per cent) were reporting menses and were theoretically eligible for " rhythm advice". But such advice was not given to 135 of these 247 women for reasons given below. The rhythm method was actually taught to *112 couples*—to 14 per cent of those willing to learn. The remaining 699 couples either became unwilling or proved unsuitable for several reasons. Some of these reasons are similar to those responsible for the exclusions already noted, and have nothing to do with the rhythm method. But others—and these are in a large majority —are directly connected with difficulties and defects presented by the rhythm method.

The *Report* does not distinguish between these two groups of reasons, which is perhaps a pity. In what follows an attempt will be made to separate them.

Rhythm Method Not Taught : 699 among 811 " Willing to Learn " Couples

The 699 couples who either became unwilling to learn, or proved unsuitable for, the rhythm method are classified by the report in the following five groups :

> *Rhythm method not taught: 135 couples*
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> These women were eligible for " rhythm advice " in the sense that they were reporting menses. But two were temporarily away from the village and the remaining 133 reported either an insufficient number of cycles to be eligible for instruction (ninety women) or they reported three or more cycles with too great a variation in their length to be suitable for the rhythm method (forty-three women). All these women were eligible to be taught the routine appliance method, which only needs one or two sessions of instruction.

Women pregnant: 83 women

Of these eighty-three women, seventy-six became pregnant during the six months " observation period " required before the rhythm method was taught. Seven became pregnant without menstruating. At least seventy-six out of these eighty-three cases could theoretically have been taught an appliance method before they became pregnant.

Women not menstruating: 312 women

Lactation was the cause in 296 women. The failure of the remaining sixteen to menstruate was either due to illness or to the fact that they had recently lost young children or had had stillbirths. All these women were theoretically eligible to learn an appliance method. This subgroup of women is easily the largest of the five.

Unwilling to learn: 98 couples

" Shortly after the Instruction stage of the study got under way ", the *Report* says, " a number of couples began to change their minds and became unwilling to learn the method." The causes of this change are discussed at some length. The reasons are generally similar to those given by the 277 couples (a quarter of the total) who, in the attitude survey, declared themselves unwilling to learn a method. Among these reasons a desire for more children and a belief that no more children would be born figure prominently. None of the reasons is specifically related to the rhythm method.

Closed cases: 71 women

The causes of closure are closely similar to the causes of the abandonment of the attitude surveys (213 cases) above noted. The commonest (thirty-five cases) was that the couple had moved or had been separated from each other (in six cases by death); and that the wife had reached the menopause (twenty-seven women). None of these causes is specifically connected with the rhythm method.

If the inferences above drawn about the eligibility of the first three of the above five groups of women to learn an appliance method is justified, about 500 of the 699 women who could not be taught the rhythm method could have been taught another method. Between seventy and eighty of these women (those who became pregnant) could have learned a method which might have prevented an unwanted pregnancy. But it should be noted that the majority of women who might have been taught an alternative method would not have been eligible subjects for the basic inquiry. Thus 312 of the 699 women could not be taught the rhythm method because they were lactating. These women might have been taught an appliance method which would have served them when they again began to menstruate or when their periods of lactation were over —we are told that many women were menstruating while lactating. (We are also told that, in the attitude survey, many women declared that they did not begin to menstruate again till their child was eighteen months old). But the experiences of these lactating women would have been of little use for the purposes of the survey which was primarily concerned with the protection afforded by the rhythm method to women who were exposed to the risk of pregnancy.

Rhythm Method Taught: 112 among 811 " Willing to Learn " Couples

It was above mentioned that, of the 811 women who expressed willingness to learn a method, the rhythm method was actually taught to but 112. What were the experiences of these 112 women? It will be seen that a further whittling down of numbers occurred.

Method Abandoned or Outcome Unknown: 32 women

The follow-up inquiry lasted thirteen months —from January 1953 to February 1954. By the end of this period, information was lacking about two women, and two more became ineligible because their cycles became irregular after they had been given advice. There remain twenty-eight women who abandoned the method. We are given particulars about these women. Four gave up the method because of the menopause, the death of a husband, and lactation. None of these causes is specifically connected with the rhythm method. Another ten gave it up for miscellaneous reasons: five because they wanted more children and another five for reasons of which one was specifically connected with the rhythm method (" unable to abstain ") and of which the other four may have been so connected (in two cases criticism—perhaps arising from the home visits—was feared and in the other two co-operation was lacking). In the remaining fourteen cases the woman became pregnant. One of these pregnancies was wanted; the other thirteen were, in one way or another, attributable to defects of the rhythm method. Two women became pregnant when following the method regularly, eight reported coitus during

" baby days ", one felt that " people were making fun of them ", one experienced irregular cycles and did not receive the revised rhythm advice, and one was incorrectly advised by the worker. Hence the abandonment of the rhythm method was attributable in at least half of these thirty-two cases to defects specific to the rhythm method.

There remain *eighty couples* among the 112 who were taught the rhythm method. Of these half—thirty-nine couples—were, by February 1954, using the method irregularly and the other half—forty-one couples—were using it regularly.

Method Followed Irregularly: 39 women

We are not told much about these. At least seven induced abortions were known to have occurred among them.

Method Followed Regularly: 41 women

Of these we are given particulars of thirty-nine. It is on the experience of these thirty-nine women, to whom we are finally whittled down, that an assessment of the reliability of the rhythm method is possible. How far was the rhythm method successful in controlling or postponing pregnancy among these thirty-nine women? A useful standard based on the general experience of couples in the Ramanagaram area is given. Outside the fourteen experimental villages, no attempt is made to control conceptions but " knowledge about abortions is widespread ". We are told that the average period between live-births occurring to women *who make no use of birth control* methods is 42 months–3½ years. " Deducting nine months for full-term pregnancies ", the *Report* tells us, " and allowing for miscarriages and still-births, there are roughly 30 months between conceptions. About ten months should be allowed for the lactation period and the resumption of regular menses. *Twenty months* of exposure per conception remain." (The figure might be less if due allowance were made for abortions.) To what extent did the rhythm method, as regularly used by these thirty-nine women, prolong this period of exposure? The answer provided by the *Report* is not precise. We are told that, by February 1954, when the experiment was wound up, two of these thirty-nine women had become definitely pregnant and that there were four further suspected pregnancies. It would surely have been a small task, in view of the scale of the experiment, with its fifty or so workers and sixteen control villages, to have definitely ascertained whether these four women were or were not pregnant. But for some reason this was not done. Hence the important question of the reliability of the rhythm method turns on suppositions. On the assumption that only two of six of the thirty-nine women who used this method regularly were in fact pregnant, the months of exposure per conception per couple is given as 110·5, or five and a half times the average expectation (20 months) for women in the Ramanagaram area who use no birth control; on the assumption that the four suspected pregnancies were real (six out of the thirty-nine women having become pregnant), the months of exposure are reduced to 35·8 or a little less than double the normal expectation. The nearest, therefore, that the report on women in the Ramanagaram experiment can take us to the desired assessment of reliability is the statement that " the regular users of the method can hope to postpone the next pregnancy from nearly 2 to 5½ times as many months of exposure as those who do not use the method, if the above computations remain valid for a larger number of couples for a longer period of time ". (It may here be mentioned that the figure for women in the Lodi experiment who regularly used the rhythm method was 53 months of exposure, which is roughly intermediate between the two outside figures (110·5 and 35·8) for the reported thirty-nine users of rhythm in the Ramanagaram experiment).

Difficulties and Defects of Rhythm Method

These are well conveyed by a revised enumeration of the 811 " willing to learn " couples which is contained in a final summary.

	Numbers	Per cent
1. Couples following the method: Regularly 41, Irregularly 39, Unknown 2 ..	82	10·1
2. Couples with too great variation in the woman's cycles for using the method	47	5·8
3. Couples reporting insufficient cycles for teaching the method	90	11·2
4. Couples with wife pregnant. (At least 76 of the women here became pregnant during the observation period of six months, i.e. after reporting one onset date) ..	97	11·9
5. Couples unwilling to learn or to use the rhythm method	108	13·4
6. Couples with wife not reporting menses (at least 296 of these were lactating)	313	38·5
7. Closed cases: couples no longer available	74	9·1
	811	100·0

Groups 2 to 6 include women who could not be taught the rhythm method because of difficulties and defects which are characteristic of that method. These five groups comprise 655 out of the 811 " willing to learn " women (over 80 per cent). The great majority of these 655 women could have been taught another method. The lactating women contained in group 6, if taught another method which might have protected them advantageously in the future (when they began to menstruate again or ceased to lactate), could not have contributed

much information to the two main issues of the experiment, namely the acceptability and reliability of a method. These women could have made a modest contribution to the acceptability problem by their reactions to an alternative method and their willingness to learn it; but they could not put the method into practice as long as they continued to lactate: and they could provide no information, as long as they were not menstruating, on the method's reliability.

Ramanagaram Experiment: Summary of Figures

For purposes of comparison with the Bannikuppe and Lodi experiments, accounts of which follow, a summary of the figures above set forth may be helpful.

Total households in the fourteen experimental villages of Ramanagaram.. .. 1,600

Number of couples in these 1,600 households who were (supposedly) sexually active, the wives being under forty 1,301

Number of couples, among the 1,301 as to whom attitude surveys were completed.. 1,088

Number of couples, among the 1,088, who were willing to learn a method of birth control .. .. .. 811 (75 per cent)

Number of couples, among the 811, where the women reported menses .. 247 (30 per cent)

Number of couples, among the 811, where the women reported menses and to whom the rhythm method was taught 112 (14 per cent)

Number of couples, among the 112, who, at the end of the experiment, were regularly using the rhythm method .. 41 (37 per cent)

Number of months exposure per conception per couple among thirty-nine of forty-one regular users of rhythm (the average number of months of such exposure for women not practising birth control in the area of Ramanagaram being 20)—a figure somewhere between 35·8 and 110·5.

The One-Cycle Rule: Bannikuppe

Something should briefly be said about this relatively small experiment which was begun late in July 1953 at the request of the village leaders and which, in respect of the *acceptability* seems to have been more successful than the main experiment. Bannikuppe is a fifteenth village in the experimental area of the Ramanagaram project and is larger than any of the other fourteen. It contains nearly fifteen hundred people.

The figures corresponding to those just given are as follows:

Households in the village 231

Attitude survey included 181 couples and was completed for 164 couples.

Number of couples where one or both partners wished to learn a method.. 123 (75 per cent)

Of these 123 couples, the wife was pregnant in twenty-nine, and neither pregnant nor menstruating in sixty; thirty-four (28 per cent) of the 123 wives reported menses and were therefore eligible recipients of "rhythm advice".

By the end of February 1954, seven months after the Bannikuppe experiment began, no less than thirty-three out of these thirty-four couples had learned the One-Cycle rule, and, of these thirty-four couples, twenty-four were using it regularly. The remaining nine were using it irregularly or had given it up for various reasons which included one case of pregnancy and one in which, in addition to the One-Cycle rule, the couple were practising coitus interruptus during the baby days.

Even when due allowance is made for the short period during which the Bannikuppe experiment lasted, the figure of twenty-four regular users of the rhythm method among thirty-four to whom it had been taught compares favourably with that for the main experiment—forty-one regular users among 112 such women.

The report comments on the following favourable features of this small experiment: at the outset Bannikuppe couples were familiar with the idea of the rhythm method and asked for it to be taught to them; the workers had gained in skill by the time the experiment began; it was advantageous to be able to dispense with beads and calendars and still more so with the long "observation period" of six months; the period of fifteen baby days imposed by the One-Cycle rule was more acceptable than the longer period of about twenty-one days during which couples in the fourteen other villages were required to practise abstinence for the first three months after rhythm advice was first imparted (from the fourth to the seventh onset day); and lastly fewer home visits

were necessary, thus conserving worker's time and avoiding possible resentment. We are told that many couples in the other villages " did not wish their neighbours to know that they were learning the rhythm method and resented the great many visits necessitated during the instruction stage ".

The reader is left in no doubt that the One-Cycle rule, as taught at Bannikuppe, is more acceptable to the Indian villagers of the Ramanagaram area than the routine method as taught at the other fourteen villages. But whether the One-Cycle rule is as reliable as the other is another matter. The smaller experiment did not last long enough for its reliability to be assessed, and in any case the data as to reliability of the routine method as reported in the other fourteen villages are too meagre to provide a standard of comparison.